The + and − below show the mean error in that digit to exceed or fall short of the mean of all (1·44). The readings are on a 1-inch space. Thus it is apparent that the errors exceed the mean in the four middle digits, and fall short of it in the others with but one exception.

Now, the practical results which may be deduced from these data are as follows :—

From (1), that there is considerable variation in accuracy of estimation; yet, in persons fit to estimate, it may be put at $d \div 50$ up to $d \div 100$; and therefore as the maximum error in some hundreds of readings is only about three times the average error, it is useless (and, worse than that, confusing) to have divisions smaller than 20 × the quantity required to be read. The application of this result to surveyors' rods, &c., would be very conducive to ease in distant reading.

From (2), that for ordinary eyes about $i \div 2000$ is the mean error of reading (with one eye only), and therefore that $i \frac{80}{2000}$ is the smallest size useful for reading with the naked eye; consequently any closer divisions than these ($\frac{1}{25}$ of an inch) are detrimental because of the greater confusion they introduce.

From (4), that in small divisions no consideration is necessary, as the first instinctive impression is nearly, or quite, as accurate as careful estimation.

From (5), that it is important not to look at a space askew; and that if unavoidable, careful consideration is desirable.

From (6), that in reading a series of observations of one amount, the right and left eyes should be used alternately to equalise the + and − errors.

From (8), that the right eye should be used in preference to the left.

From (9), that both eyes should be used wherever possible.

From (10), that red is the worst colour for measures, and that green or blue are as good as white; so that blue and white would probably be the best practically, and more distinct from vegetation, &c., than the green at a distance.

From (11), that more care is required in readings in the central portions of a space than at the ends. This is as true on spaces of $i \div 41$ as on $10i$ spaces.

Of course some of these results may be *personal* errors, and not generally true, but at least this notice will perhaps call the attention of those accustomed to estimation to the subject, and so elicit some definite statistics. There are many other interesting questions which I have not touched on here as being of less consequence, and not affecting practical questions so much as the foregoing results; these results it is to be hoped may lead to a better acquaintance with a subject of such practical importance.

W. M. FLINDERS PETRIE

Bromley, Kent

The late Dr. von Willemoes-Suhm

IN the list of papers given in Prof. Wyville Thomson's obituary of the late Dr. von Willemoes-Suhm (NATURE, vol. xiii., p. 88), is omitted—"Notes on some young stages of Umbellularia, and on its Geographical Distribution;" ("Ann. and Mag. Nat. Hist.," 4th ser., vol. xv., p. 312, and pl. xviii. A.) This animal is a Pennatulid, and was obtained during the *Challenger's* cruise at depths from 1,375 to 2,600 fathoms. It is usually associated with decidely deep-sea forms.

To this may be added two papers which were read at the meeting of the Royal Society on Dec. 9th, "On the Development of *Lepas Fascicularis*, and the '*Archizoëa*' of Cirripedia;" and "Preliminary Remarks on the Development of some Pelagic Decapoda."

J. C. G.

Seasonal Order of Colour in Flowers

I HAVE often noticed during spring and summer that flowers appear to follow the spectrum from the blue of the wild hyacinth to the deep scarlet of our summer flowers. Will any of your correspondents tell me if during spring the actinic rays of the sun are in greater force, and whether these chemical rays are those which revivify seeds and plants after their winter's hibernation? During autumn I have noticed the same gradations in the colours of flowers.

C. E. HERON ROGERS

Retford, Notts, Dec. 7

OUR ASTRONOMICAL COLUMN

ATLAS PLEIADUM, Σ 453.—Has anyone provided with a large telescope examined this object of late for duplicity? It appears very improbable that Struve's epoch of 1827 can have been put upon record through an illusion. We find in "Mensuræ Micrometricæ," p. 2 :—

1827·16 Position 107°·8 Distance 0″·79 Mag. 5 and 8.

It is then termed "duplex difficillima." On subsequent examination, 1830·25, the position was found to be 29·2, and the distance only 0″·35. In 1831 and 1832 it was not seen double with the Dorpat refractor under a power of 600. Struve remarks, "si justa est anni 1827 notatio, quod vix dubitare potest, cum nullum observans dubium expresserim, motus in hoc systemate egregrie celer inesse debet." From 1840 to 1847, Mädler observing with the same instrument, says he always found the star "quite round," and to our knowledge a few years since the star was pronounced single after close scrutiny with more than one of the larger telescopes.

THE "GEGENSCHEIN" OF THE ZODIACAL LIGHT.—The last number of the "Monthly Notices of the Royal Astronomical Society" contains some interesting remarks by Mr. Backhouse, of Sunderland, bearing upon this phenomenon of the zodiacal light, as observed by him between the years 1871 and 1875, which are the more notable, as the observer does not appear to have been aware of its previous observation, and consequently the particulars he has placed upon record afford a very satisfactory confirmation of the existence of this feature of the light. The "gegenschein" consists in an elliptical patch of light almost directly opposite the sun's place, with the greater axis nearly in the plane of the ecliptic. Attention was first directed to it in the year 1854 by Herr Brorsen (the discoverer of the comet of short-period—February 1846)—while observing at Senftenberg in Bohemia. It may be inferred that it is generally difficult to distinguish or trace, with certainty, as even Prof. Heis, of Münster, with his excellent vision and long practice, did not succeed in verifying Brorsen's observations for several years (Zodiacallicht-Beobachtungen, Heis, 1875). Observations of the phenomenon will be found in various numbers of the "Astronomische Nachrichten." Brorsen thought this opposition-glow of the zodiacal light was more distinctly seen near the vernal than the autumnal equinox; the observations of Heis are in January, April, and in December. There are also observations by Schmidt at Athens, Schiaparelli at Milan, &c.

THE COMET OF 1729.—This very remarkable body presented a case which is quite unique in the history of these interesting denizens of the solar system. It was visible without telescopic aid, or with little optical assistance, at distances from the earth and sun not greatly inferior to those to which any other comet has been followed even with such an instrument as the Poulkova refractor, and it may be inferred was of very different constitution.

The comet was first detected by Sarabat, an ecclesiastic of the Jesuit order, at Nismes, on the 31st of July; the moonlight, which was then on the increase, prevented his seeing it on the following evenings until Aug. 8, when the moon being totally eclipsed, he recovered the comet and gave intimation of his discovery to Cassini at Paris, by whom it was observed on forty-four nights between Aug. 31 and Jan. 18 following. Cassini saw the comet without fixing its position until the end of the same month. While it was visible with difficulty to the naked eye, in a 16-feet telescope it appeared "en forme d'une petite étoile nébuleuse avec une chevelure autour d'elle, dont l'étendue paraisait au moins aussi grande que le diametre de Jupiter, vu avec une pareille lunette." This is about the extent of our information relating to the aspect of the comet.

The best orbits are those of Burckhardt (hyperbola and parabola), published in the "Connaissance des Temps" for 1821. If we employ the parabolic elements we find the following distances of the comet from the sun and earth expressed in units of the earth's mean distance from the sun :—

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12^h G.M.T.	Distance from Sun.	Distance from Earth.
1729, July 31	4·065	3·121
,, Aug. 8	4·072	3·134
1730, Jan. 18	4·452	5·152
,, ,, 31	4·499	5·225

For comparison with these distances take the case of the great comet of 1861, which was observed by Mr. Otto Struve at Poulkova till 1862, May 1, when its distance from the sun was 4·46, and from the earth 4·70; or that of Mauvais's comet of 1847, followed by Bond with the great refractor at Cambridge, U.S., till 1848, April 21, and then distant from the sun 3·85, and from the earth 4·40; or again, the case of the celebrated comet of 1811, last seen by Wisniewsky at Novo-Tcherkask, 1812, August 17, when the radius-vector was 4·54, and the distance from the earth 3·50.

RÜTIMEYER ON TRACES OF MAN RECENTLY DISCOVERED IN THE INTERGLACIAL COAL-BEDS OF SWITZERLAND

THE last number of the *Archiv für Anthropologie* contains a short but important article by the eminent zoologist of Basle, Prof. Rütimeyer, on some traces of man recently discovered in the interglacial coal-bed at Wetzikon. Escher von der Linth first called attention to the fact that at several places in East Switzerland, especially on the eastern shore of Lake Zurich from Wetzikon to Utznach, and again in the neighbourhood of the Lake of Constance, there are beds of coal, which are not only covered by, but which also repose on, well-marked glacial deposits, thus clearly proving the existence of more than one period of extreme cold, as first remarked by Morlot, and since confirmed by many observers, and especially by Geikie.

These interglacial coal-beds contain numerous remains of plants and animals, among the most interesting being those of *Elephas antiquus* and *Rhinoceros merkii*. The remains of plants are indeed so numerous that Dr. Scheuermann, of Basle, has been in the habit of breaking up himself all the coal used as fuel in his house, in search of vegetable remains. In doing so he was struck on one occasion by observing a number of pointed rods lying side by side, and he placed the block of coal containing them in the hands of Prof. Rütimeyer.

Prof. Rütimeyer has now given a description and figures of these rods, from which it is clear that they have been intentionally pointed, and that they formed a portion of rough basket or wattle work. They are four in number, and are closely embedded in the coal, which they precisely resemble in colour, while the texture is that of the ordinary wood found in these coal-beds. Moreover, as is usual in such cases, the stem has been compressed, so that the section is not circular but oval. According to Prof. Schwendener, the wood is that of *Abies excelsa*. The points bear evident traces of cutting, while at one part of the rods are marks as if of a string wound round and round them. Here, then, we appear to have clear evidence of the existence of man during one of the warm intervals of the glacial epoch.

J. L.

THE THEORY OF "STREAM LINES" IN RELATION TO THE RESISTANCE OF SHIPS[*]

III.

IN this treatment of the propositions concerning the flow of fluid through pipes, I have at length laid the necessary foundation for the treatment of the case of the flow of an infinite ocean past a submerged body. I have shown these propositions to be based on principles which are undeniable, and the conclusions from which, when in any way startling or paradoxical, you have seen confirmed by actual experiment.

I have dealt with the case of a single stream of uniform sectional area (and therefore of uniform velocity of flow) enclosed in a pipe of any path whatever; I have dealt with the case of a single stream of very gradually varying sectional area and velocity of flow; and I have dealt with the case of a combination (or faggot, as it were) of such streams, each to some extent curved and to some extent varying in sectional area, together composing the whole content of a pipe or passage having enlargements or contractions in its course; and in all these cases I have shown that, provided the streams or pipe-contents finally return to their original path and their original velocity of flow, they administer no total endways force to the pipe or channel which causes their deviations.

I am now going to deal with a similar combination of such streams, which, when taken together, similarly constitute an infinitely extended ocean, flowing steadily past a stationary submerged body; and here also I shall show that the combination of curved streams surrounding the body, which together constitute the ocean flowing past it, return finally to their original direction and velocity, and cannot administer to the body any endways force.

The argument in this case is, in reality, precisely the same as that in the case of the contractions and enlargements in pipes which I have already dealt with; for, in fact, the flow of the ocean past the stationary submerged body is only a more general case of the flow of fluid through a contracted pipe; but, though the cases are really the same, there is considerable difference in their appearance; and therefore I will proceed to point out how the arguments I have already used apply equally to this case.

Every particle of the fluid composing the ocean that passes the body must undoubtedly follow some path or other, though we may not be able to find out what path; and every particle so passing is preceded and followed by a continuous stream of particles all following the same path, whatever that may be. We may, then, in imagination, divide the ocean into streams of any size and of any cross-section we please, provided they fit into one another, so as to occupy the whole space, and provided the boundaries which separate the streams exactly follow the natural courses of the particles.

I before suggested a similar conception of the constitution of the ocean flowing past the stationary body, and there pointed out that the streams forming this system must not only be curved in order to get out of the way of the body, but might each require to have to some extent a different sectional area, and therefore a different velocity of flow at different points of their course. If we trace the streams to a sufficient distance ahead of the body, we shall there find the ocean flowing steadily on, completely undisturbed by, and as we may say ignorant of, the existence of the body which it will ultimately have to pass. There, all the streams must have the same direction, the same velocity of flow, and the same pressure. Again, if we pursue their course backwards to a sufficient distance behind the body, we shall find them all again flowing in their original direction; they will also have all resumed their original velocity; for otherwise, since the velocity of the ocean as a whole cannot have changed, we should have a number of parallel streams having different velocities and therefore different pressures side by side with one another, which is an impossible state of things.[*]

Although, in order to get past the body, these streams follow some courses or other, various both in direction and velocity, into which courses they settle themselves in virtue of the various reactions which they exert upon one another and upon the surface of the body, yet ultimately, and through the operation of the same causes, they settle themselves into their original direction and original velocity. Now the sole cause of the original departure of each and all of these streams from, and of their ultimate return to, their original direction and velocity, is the submerged stationary body; consequently the body must receive the sum total of the forces necessary to thus affect them. Conversely this sum total of force is the only force which the passage of the fluid is capable of administering to the body. But we know that to cause a single stream, and therefore also to cause any combination or system of streams, to follow any courses

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[*] Address to the Mechanical Section of the British Association, Bristol, August 25, 1875; by William Froude, C.E., M.A., F.R.S. President of the Section. Revised and extended by the author. Continued from p. 93.

[*] In an imperfect fluid it *is* possible to have parallel streams having different velocities and the same pressures side by side with one another, because, in an imperfect fluid, change of velocity may have been communicated by friction instead of by difference of pressure.

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